Review of HESS Manuscript #2019-153, revised version
Title: Impacts of non-ideality and the thermodynamic pressure work term pΔv on the Surface Energy Balance
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Review by A. S. Kowalski

This revised manuscript is much improved in comparison with the initial submission, and I suggest that it be published subject to minor revisions. I have tried to organize my recommendations in order of importance.

1. Line 245: The author claims to identify an error in equation (2.66) of Curry and Webster (1999), but I see no error in that equation. The coefficient 0.2 modifying q_v derives from a binomial expansion and approximation, multiplying both numerator and denominator by the same factor (1 - 0.*87* q_v) and then neglecting quadratic terms to simplify the result (since $q_v^2 << 1$). This can be found in other texts as well (e.g., Rogers and Yau, 1988, A Short Course in Cloud Physics, Pergamon, Oxford). In equation (15) and all subsequent equations that contain the factor 0.33, I believe that this should be changed back to the coefficient 0.2. This may also change the percentage that appears in the conclusions (line 298).
2. Line 284: The "displacement assumption" of Pau U et al. (2000) can hardly be brought into question, since it falls directly out of the Ideal Gas Law for the conditions that they assumed. The context of the Paw U et al. paper is evaporation that is both isobaric (as assumed by the Webb et al. paper under consideration) and isothermal (excluding temperature effects – i.e., the WPL vapour correction). In such a context, equation (4) of Webb et al. (1980) is a version of the Ideal Gas Law that adequately justifies the relevance of water vapour displacing dry air. I appreciate the author's argument that evaporation is truly neither a constant volume, nor a constant pressure process (line 103), but I do not think that it justifies the wording used here.
3. Lines 230-240: There is something inconsistent about beginning an argument for defining the heat flux using potential temperature (rather than the temperature) with an equation that is valid only for an *incompressible* atmospheric process. It may be preferable to use the proper definition of the material derivative as $\frac{d\theta}{dt} = \frac{\partial\theta}{\partial t} + \boldsymbol{u} \cdot \boldsymbol{\nabla}\theta$ and so be able to remove the word "incompressible". Perhaps even simpler would be to simply state that the potential temperature is the key variable for discussion, and cite an appropriate reference (e.g., Kowalski, A. S. and Argüeso, D., 2011, *Tellus*, **63B**, 1059-1066).
4. I find the author's use of temperature ranges to be inconsistent and frankly inexplicable, resulting in an unnecessary distraction from the message of the paper. Line 57 suggests examination of the surface energy budget near STP (i.e., not far from 0ºC), which seems appropriate if somewhat vague. However, temperature ranges are later defined variously throughout the manuscript (all converted to ºC here) as:
    a. Line 59: 0 – 100ºC;
    b. Line 155: 3 – 42ºC;
    c. Line 185: 7 – 77ºC; and

d. Line 298: 12 – 52ºC.

I believe that a more appropriate range of “temperatures commonly encountered with micrometeorological techniques” (line 156) would be something like -35º - 45ºC. If the “extrapolation” of Dr. Massman’s results to such a range in any way changes his calculations, then some revision may be required that might not classify as “minor”.

5. The use of both mass- and molar-based definitions of the specific heat is similarly distracting. I see little point in defining the molar specific heat when its use complicates the “final result” (as in equation 13 which, if I am not mistaken, has disguised the mass specific heat as the ratio c_v/μ).
6. Throughout the manuscript, units are specified with no space separating them. So for example at line 16, I think that “Wm^{-2}” should be changed to “$W\ m^{-2}$”, and likewise in many subsequent instances. This is particularly egregious at line 43, where the characters “kgm” appear in succession.
7. At line 69, delete the first instance of the word “pure”.
8. At line 72, “unnecessary **to** consider”
9. At line 75, “components of the **specific** enthalpy”

I hope that some fraction of these suggestions will be helpful when producing the final version of the manuscript.